NOTES AND LITERATURE

MIMICRY

In some ways it would be a pity if the theory that mimicry has arisen through the operation of natural selection must be discarded since it is so ingenious in itself and was originated and fostered by such masters of theoretical biology. However, the old order seems to be surely giving place to new, here, as in other phases of the study of evolution. Since Wallace's "Papilionidæ of the Malayan Region" the case of *Papilio polytes* has been a classic. The females of this butterfly are of three sorts: one like the male *polytes,* one like *P. aristolochiæ* and the third like *P. hector.* The two latter species are supposed to be distasteful to insectivorous animals while *P. polytes* is supposed to be edible. The two "models" are numerous in individuals and while "*P. hector* and the *hector* form of *P. polytes* are confined to India and Ceylon, both *P. aristolochiæ* and the *aristolochiæ* form of *P. polytes* have a wider range eastward." The case is complete and has been convincing.

However, Punnett[1] found that in Ceylon

> The following statements may be taken as a fair presentation of the facts:
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> 1. In the low-country the male form of *polytes* female is at least as numerous as either of the other forms, and may be the most abundant of the three.
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> 2. In the northeast of the island, in the hector country, the aristolochiæ form *polytes* is nearly as abundant as the hector form, though its model is at any rate exceedingly scarce.
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> 3. Higher up-country, where *P. hector* is rare or absent and *P. aristolochiæ* is common, the hector form of *polytes* is more abundant than the aristolochiæ form.
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> It is obvious that these statements are not in harmony with the ideas of those who look to the theory of mimicry for an explanation of the polymorphism that exists among the females of *P. polytes.*

His observations concerning the enemies of butterflies confirm those of other heterodox students, namely: that "as serious enemies of butterflies in the imago state birds may be left out of

[1] "Mimicry in Ceylon Butterflies, with a Suggestion as to the Nature of Polymorphism," *Spolia Zeylanica,* Vol. VII, Part XXV, September, 1910.

account," that lizards "certainly do not appear to exercise that nice discrimination with regard to butterflies which is necessary for the establishment of mimicking forms on the theory of natural selection," and that asilids are not averse to preying upon "distasteful species."

After pointing out that the resemblances on which the theory was based are far less striking in living, moving specimens than in their expanded museum state, he says

> Apart then from the questions whether the resemblances in many cases of mimicry are sufficiently close to be of effective service to the mimic, and whether the action of natural selection can be regarded as sufficiently stringent to have brought these resemblances into being, there are still the following difficulties in the way of the acceptance of the hypothesis of those who look to natural selection as an explanation of polymorphic forms in Lepidoptera:
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> 1. The attribution of selection value to minute variation.
> 2. The absence of transitional forms.
> 3. The frequent absence of mimicry in the male sex.
> 4. The inability to offer an explanation of polymorphism, where the polymorphic forms can not be regarded as mimics of a distasteful species.
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> Moreover, the hypothesis assumes that minute variations of all sorts can be inherited, a position which at present is lacking in experimental proof.

The gist of the constructive part of his paper is as follows:

> Natural selection plays no part in the *formation* of these polymorphic forms, but they are regarded as having arisen by sudden mutation, and series of transitional forms do not exist because such series are not biologically possible. Polymorphic forms may arise and may persist, provided that they are not harmful to the species, and it is possible to look upon their existence as due to the absence of natural selection rather than to the operation of this factor. . . . That polymorphism in a species should so frequently be confined to the female sex has long been remarked upon by those who study these matters, and the explanation most favored is that the female, burdened as she is with the next generation, is more exposed to the action of natural selection and in greater need of some protective adaptation. The weak point of such a view is that it does not explain why the male is not similarly protected. In connection with this problem recent Mendelian research on sex-limited inheritance is highly suggestive. It has been shown that certain types of inheritance receive their simplest explanation on the assumption that the female is heterozygous for a sex factor not contained in the male and that this sex factor may, on segregation of the gametes, repel the factor for some other character for which the female is also

heterozygous. From the beautiful experiments of Doncaster and Raynor it has been inferred that inheritance of this type occurs in the common currant moth (*Abraxis grossulariata*), where a distinct color variety, var. *lacticolor,* occurs. The factor for *grossulariata* pattern appears to segregate against the female sex factor, with the consequence that in only one type of mating, and that a rare one, is the *lacticolor* pattern transmitted to the male sex.

Gametic formulæ are suggested and the conditions they impose are mentioned, but no breeding work was done. Whether the above explanation of the behavior of *grossulariata* is correct or not and also the correctness of the suggested formulæ for *polytes* are immaterial to the present discussion. It is now well known that "mutations" do occur in the females of insects and that the new characters can be transferred to the male by proper breeding. But, why do the mutants of *P. polytes* resemble greatly, even if they do not do so to such an extent as had been supposed, other species? On account of similar anatomical and physiological make up; or, in this case, did the proper gametic couplings once take place so that the then new female type was transferred to the males (as in *grossulariata*) and was thereafter continued with such other modifications as were necessary to separate them taxonomically? In other words, the mimicking species came first and gave rise to the model!

Mutation, in itself, is not the whole story. Granting it, we must be given a reason for the mutant resembling something else and while the amendment just made to Punnett's paper *may* carry for this case, the chances are against it and we can not apply it to resemblances between species of different orders. In this connection, however, there seems to be an important thing which is often overlooked. It would be far more wonderful if, among the thousands of new forms which have arisen, there were no resemblances than it is that some of the forms are very much alike.

As Punnett and others have pointed out, the same process which brought about such a close resemblance between, for example, earwigs (Orthoptera) and rove beetles (Coleoptera) that they are frequently mixed in entomological collections doubtless caused also the resemblances (here called mimicry because an advantage can be imagined) between certain flies and certain stinging Hymenoptera. If "chance" or "environment" is used in the former case it is not unlikely that it applies in the latter also.

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